A Replication of “Counting Chickens When They Hatch”

(Economic Journal, 2012)

David Roodman

March 2014

Abstract. Clemens, Radelet, Bhavnani, and Bazzi (CRBB, 2012) argues that the most-cited cross-country studies of the impact of foreign aid can be reconciled if changed in certain ways. The shared finding is then, in the CRBB view, that more aid is followed on average by more growth. I exactly replicate the estimation results of CRBB, and then question these estimation results with additional specifications and tests. One change, restricting the aid variable to assistance most likely to affect growth in the short run, does not make much difference. Two other changes—lagging the aid variables and differencing the data to remove fixed effects—fail to remove contemporaneous endogeneity. Addressing this issue produces evidence of zero or negative Granger causation from aid/GDP to growth. And the regressions embodying all of CRBB’s changes do not peg the impact of aid as statistically significant.

Keywords: Foreign assistance, cross-country growth regressions, replication

JEL codes: F35, O11, O40

INTRODUCTION

“Counting Chickens When They Hatch” by Clemens, Radelet, Bhavnani, and Bazzi (CRBB, in *Economic Journal*, 2012), unifies influential studies of the impact of foreign aid. As CRBB documents, the cross-country aid-growth literature has split over whether aid appears to “work” in the sense of expanding economic activity on average; or does not work; or works only under certain circumstances, such as good macroeconomic management. CRBB argues that various entries in this literature will, after certain changes rooted in economic and econometric theory, converge to a similar result: “aid inflows are systematically associated with modest, positive subsequent growth.”

I exactly replicate the estimation results of CRBB, and then question these estimation results with additional specifications and tests. I find the results less compelling than CRBB as evidence of the impact of aid on growth. The reasons appear to be five:

- For each of the three earlier papers that CRBB reanalyzes, it runs a variety of specifications. CRBB looks for consistent findings across specifications. In contrast, I focus on the most rigorous CRBB specifications, the ones embodying all the CRBB-advocated changes. In doing so, I argue, I apply CRBB’s standards to CRBB’s results.
- Though not discussed in CRBB, those most rigorous CRBB regressions are vulnerable to contemporaneous endogeneity of aid to growth.
- One CRBB change—narrowing the aid variable to forms of assistance that can be expected to raise growth within a few years—does not make much difference. This cuts against the CRBB suggestion that other studies fail to detect the impact of aid on growth for failure to track the right subflow.
- The CRBB preferred regressions model the impact of aid as parabolic. CRBB synthesizes the estimated coefficients on aid/GDP and (aid/GDP)² by reporting the aid level at which the aid-growth association peaks, but not the magnitude of the association at that level. In language that is simpler but laden with causal connotations, CRBB reports the *aid level of maximum impact*, not the *maximum impact*. The latter is much more relevant to the conclusion and usually closer to zero statistically.¹
- The mildly positive association reported in CRBB between aid/GDP and growth one period later is offset by an association with growth two periods later that is mildly negative and more plausibly exogenous.

This paper reviews the approach of CRBB; replicates and extends its most rigorous specifications; documents and expunges a source of contemporaneous endogeneity; discusses and works to fix additional econometric issues, which prove secondary; and concludes.

CRBB IN THUMBNAIL

Rather than building a fresh data set and specification, as in Clemens, Radelet, and Bhavnani (CRB, 2004), CRBB reanalyzes the three most-cited studies in the aid-growth literature: Boone (1996), Burnside and Dollar (BD, 2000), and Rajan and Subramanian (RS, 2008). Boone and RS come from what CRBB terms the “null strand” of the literature, which cannot detect any impact of aid on growth of investment/GNI (Boone) and GDP/capita (RS). BD leads the “conditional strand,” finding that aid works in countries with “good” economic policies.²

For each subject paper, CRBB begins with pooled panel Ordinary Least Squares (OLS) regressions using the subject’s controls, sample, and data set—in the case of Boone, rebuilt from scratch. CRBB then adds more years of data and introduces specification changes stepwise. All data sets examined are panels; Boone’s period length is 10 years, BD’s 4, and RS’s 5.³ CRBB’s preferred specifications difference all variables and regress on lagged aid. These choices generate demand for prior-period observations that shorten the regression timeframes—to 1991–2000 for Boone, 1978–2005 for BD, and 1981–2005 for RS. The CRBB unification project superficially resembles Roodman (2007), which copies specification choices such as period length and control set from one aid-growth paper to another. But the CRBB strategy is nearly orthogonal in introducing specification changes largely absent from the subject studies. A core theme in CRBB is the need to factor into study design the likely time profile of aid’s impacts. In reanalyzing Boone, BD, and RS, CRBB modifies specifications in several ways to reflect sensitivity to time: restricting the aid variable to subflows that can be most expected to affect growth within a few years, called “early-impact aid”; lagging the aid variable one period to combat endogeneity; first-differencing the

data to remove omitted-variable bias from fixed factors; and, since differencing renders a predetermined control, initial log GDP/capita, presumptively endogenous, instrumenting this variable with its lag (Anderson and Hsiao 1982). CRBB also introduces two more changes, which are not bolted to the paper's conceptual frame: adding more years of data; and regressing on $(\text{aid}/\text{GDP})^2$ along with aid/GDP in order to model nonlinear associations.

CRBB summarizes its results as showing a positive correlation between past aid/GDP and current growth. After some robustness tests, the paper concludes that causation of growth by aid best explains the correlation. Roughly speaking, if the three subject studies had matched CRB (2004)'s key specification choices, they would have matched CRB's results.

As none of the regressions instruments for aid, lagging is the main strategy for reducing endogeneity. Thus the notion of causality pursued is akin to that of Granger (1969). For precise interpretation, it is worth noting that regressing growth on previous-period aid/GDP effectively mixes two causal pathways; that is, aid in one period may affect aid in the next period, which may affect growth in that same period, and aid in one period may affect growth in the next through channels outside the regression model. However, since both are stories of aid impact, the distinction between them is secondary to whether causality from aid to growth is the most plausible explanation for the results.

REPLICATION

CRBB makes its changes to the three subject studies in various combinations, producing 67 regressions. Just two—one each for BD and RS—incorporate all the advocated changes.⁴ These are Anderson-Hsiao regressions on lagged early-impact aid/GDP in CRBB's extended samples. The closest CRBB comes to an Anderson-Hsiao version of the other subject study, Boone, is OLS in differences, which is inconsistent (see below). I therefore focus on those two instances of CRBB's preferred specification (columns 11 of CRBB tables 7 and 9).

The data and code for CRBB are on cgdev.org, making replication a trivial task. Table 1 displays

original and replicated results for the CRBB preferred specification. Up to CRBB reporting precision, the matches are nearly perfect.

In light of the CRBB conclusion that “aid inflows are systematically associated with modest, positive subsequent growth,” the replicated results seem weak. In the BD and RS regressions, the coefficients on the linear early aid/GDP term are significant at probability thresholds of 0.31 and 0.18 respectively. The coefficients on $(\text{early aid/GDP})^2$ are statistically stronger and negative, so they pull the estimated impact at positive aid levels even closer to zero.

How then does CRBB reach its comparatively strong conclusion? First, many of its 67 regressions generate stronger impact coefficients, which CRBB grants comparable interpretive weight: “This pattern is not statistically precise in many of the specifications, but it is either precise or close to precise in most of them.” Second, CRBB synthesizes the linear and quadratic coefficient estimates by reporting the aid/GDP level at which the apparent aid impact peaks; and this statistic is more often significant than the aid/GDP coefficients. In Table 1, the peak-impact aid level differs from zero at probability thresholds of 0.15 and 0.001.

Neither of those arguments for the CRBB interpretation is strong. Among CRBB’s 67 regressions, those replicated here are the most rigorous, the only ones that incorporate all the modifications that CRBB advocates with good reason. These regressions deserve special weight. Meanwhile, the aid level of maximum impact is less important than the *impact* at that level of maximum impact, which CRBB does not report. Put otherwise, in the parabola $y = ax^2 + bx$, the extremum is $(x, y) = (-b/2a, -b^2/4a)$; and CRBB only reports the x coordinate. The y coordinate, the maximum impact, turns out to be statistically significant much less often, as exemplified in Table 1. There, it differs from 0 only at probability thresholds of 0.53 and 0.27.

SPECIFICATION AND ROBUSTNESS CHECKS

Having confirmed a match, I perform some straightforward specification and robustness checks.

First, I adduce tests of instrument strength and serial correlation in the errors. Since the regressions instrument GDP/capita growth with lagged growth, I report the Kleibergen-Paap (2006) test for underidentification as well as the Kleibergen-Paap F statistic, which is a heteroskedasticity-robust measure of instrument strength. No studies establish critical values for the latter; CRBB suggests a rough minimum of 4 to limit bias toward OLS. For both regressions, we can easily reject the null of underidentification (see “Replication” columns of Table 2). As for the Kleibergen-Paap F measure of instrument strength, the BD regression easily clears the bar of 4 and the RS one barely does.

The Arellano-Bond (1991) serial correlation test is for second-order correlation, which is relevant for the validity of the lagged change in log initial GDP/capita—i.e., twice-lagged growth—as an instrument for the change in log initial GDP/capita. This test produces evidence of serial correlation—positive in BD and negative in RS. This calls the Anderson-Hsiao instrumentation into question. It also creates a doubt about the exogeneity of lagged early aid/GDP. And it undercuts an assumption behind CRBB’s use of standard errors that are merely heteroskedasticity-robust, not clustered by country—namely that model errors are independent across time. However, in a later section I demonstrate a change that reduces serial correlation while doing little violence to the results of interest.

Next, I check robustness to sample expansion. The CRBB data sets turn out to include complete observations for 23 additional countries for the BD regression and 22 for RS.⁵ When including these, I adjust the regressions in two other, small ways. In the RS data set, I correct an apparent error in the M2/GDP control that results in some values being high by a factor of 100. And I cluster all standard errors by country, as in CRB and RS, and essentially as in BD.⁶ (See “Revised” columns of Table 2.) This revision—mainly the sample expansion—cuts the point estimate of maximum aid impact by half or more. In the RS regression, it mildly reduces instrument strength but significantly reduces serial correlation. Overall, the sample expansion reinforces the finding that growth does not clearly follow aid.

A final test involves altering the right-side variable of interest, early aid/GDP. As mentioned, an

innovation in CRB and CRBB is the narrowing of the aid variable to assistance whose impact should manifest within a few years. CRBB operationalizes early-impact aid by counting budget support as well as investment in infrastructure, agriculture, and industry. Excluded are technical cooperation, humanitarian assistance, and most aid for social sectors such as health and education, on the idea that these will only affect growth in the long run, if at all. In ambiguous cases, such as budget support, CRBB errs on the side of inclusion, since under its prior, mixing non-early aid into early aid should conservatively bias estimates of the near-term impact of early aid toward zero.

The CRBB datasets include several aid variables as fractions of GDP: receipts of early aid for each country and period; receipts of the conventional, broader aggregate, net Official Development Assistance (net ODA); and repayments made on past ODA loans, which are what is netted out of net ODA. From these variables, I compute:

$$\text{Gross ODA} = \text{Net ODA} + \text{Repayments}$$

$$\text{Non-early impact aid} = \text{Gross ODA} - \text{Early-impact aid}$$

Table 3 shows the results of experimenting with this new variable, non-early aid/GDP. It regresses on early aid/GDP (exactly as before), on non-early aid/GDP alone, and on both at once. In the BD regressions, the linear non-early aid/GDP term (column 2) has nearly the same coefficient and standard error as early aid/GDP (column 1), while the quadratic term is much weaker, implying a maximum-impact aid level and maximum impact 5 times higher. For RS, however, the story is opposite, with little suggestion of early impact for non-early aid.

Perhaps in the BD regressions, non-early aid/GDP is merely proxying for early aid/GDP when substituted for it; but the regression on both at once (column 3) rejects this possibility by producing stable results for both variables.

The same exercise on the revised specification—the one with more countries—ends up favoring non-early aid more: in RS as well as BD, non-early aid's early impact tops out at a higher level than early

aid's. However, here too, none of the estimated impacts differs statistically from zero. These regressions do not support the hypothesis that early aid has more early impact than non-early aid. Whether this weakens the CRBB case depends on one's priors. If one's prior is that both early and non-early aid quickly increase growth, then the new results for non-early aid/GDP are confirmatory—though only mildly, since statistical significance is low all around. If one takes the view that several causal stories are competing to explain any aid-growth association—reverse causation, omitted variables—and that the CRBB causal story is distinctive in predicting that early aid is much more correlated than non-early aid with growth a period later, then a pattern of just that sort would discriminate in favor of the CRBB story. It would be a fingerprint of aid impact. But the fingerprint is not there.

The last robustness test is motivated by an econometric concern. The various CRBB estimators (OLS in levels, OLS in differences, Anderson-Hsiao as implemented) are all biased in distinctive ways, most of which CRBB notes. Here I review why, then introduce a fix for the inconsistency in the Anderson-Hsiao regressions.

A typical CRBB regression model can be cast as:

$$\Delta y_{it} = (\alpha - 1)y_{i,t-1} + \beta_1 d_{i,t-1} + \beta_2 d_{i,t-1}^2 + \mathbf{y}'\mathbf{x}_{it} + \eta_t + \nu_i + \epsilon_{it} \quad (1)$$

where y_{it} is log GDP/capita of country i in period t , d is disbursements of aid/GDP, $\mathbf{x}$ is a control set of observables, η is fixed over space, ν is fixed over time, and ϵ is a stationary, mean-zero error that is assumed to be independent at least across countries.⁷ This is a dynamic panel data model. CRBB treats $y_{i,t-1}$ as predetermined: potentially affected by growth innovations in periods $t - 1$ and earlier, but not by innovations in period t or later. CRBB views d_{it} as endogenous, making $d_{i,t-1}$ predetermined too.

Finally, CRBB treats $\mathbf{x}$ as strictly exogenous, meaning $E[\mathbf{x}_{is}\epsilon_{it}] = 0$ for all i, s, t .⁸

An immediate challenge to estimating this model is that the ν_i are “in” the regressor $y_{i,t-1}$ since equation (1) holds at $t - 1$ too. Allowing the unobserved ν_i to remain in the error of a regression will almost certainly drive that error into correlation with $y_{i,t-1}$ in (1). This positively signed endogeneity will bias

the estimate of α upward.⁹ The knock-on effects for other parameters are unclear. In this model, the nonlinearity in d but not y creates some unusual possibilities. Perhaps the relationship between v and y is nonlinear; the dependence of d on y almost certainly is since y is log GDP/capita and d is aid/GDP. The $d_{i,t-1}^2$ term could then pick up either of these linkages.

Perhaps more important, d may be endogenous to v through other channels. CRBB highlights what is essentially the fixed factor of population size (98 percent of whose variation in CRBB samples is cross-country). CRBB points out that less-populous countries receive disproportionate aid (see BD) while population size probably also affects growth through channels not controlled for in these regressions. CRBB cites Frankel and Romer (1999) on the amount of trade as a channel, Hausmann, Hwang, and Rodrik (2007) on the composition of exports, and Spolaore and Wacziarg (2005) on political integration with neighbors. The profusion of such stories again makes it unclear which way the endogeneity of $y_{i,t-1}$ to v would bias estimates of β_1 and β_2 .

The v_i are econometric nettles. Consistency requires extracting them from the error. An intuitive strategy is to introduce country dummies. But in short panels, this causes the number of estimated parameters to grow as fast as the number of countries, which also undermines consistency. Theory in Nickell (1981) and simulations in Judson and Owen (1999) show that the effect is substantial in dynamic panels with the number of periods used here. A random-effects estimator would delink the parameter count from the panel width—at the cost of assuming that the v_i are uncorrelated with aid/GDP and other regressors, that is, at the cost of assuming away the concern at hand.

In its most rigorous regressions, CRBB takes the common tack of differencing (1) to expunge v :

$$\Delta\Delta y_{it} = (\alpha - 1)\Delta y_{i,t-1} + \beta_1\Delta d_{i,t-1} + \beta_2\Delta(d_{i,t-1}^2) + \mathbf{Y}'\Delta\mathbf{x}_{it} + \eta_t + \Delta\epsilon_{it} \quad (2)$$

This step too comes at a cost, but one that is perhaps more manageable: it makes predetermined varia-

bles presumptively endogenous. For example, since in (1) Δy_{it} is endogenous to ϵ_{it} , $\Delta y_{i,t-1}$ is endogenous to $\epsilon_{i,t-1}$. The positive correlation between $\Delta y_{i,t-1}$ and $\epsilon_{i,t-1}$ can induce, with reference to (2), a negative correlation between the regressor $\Delta y_{i,t-1}$ and the error $\Delta \epsilon_{it} = \epsilon_{it} - \epsilon_{i,t-1}$. This will bias the OLS-in-differences estimate of α downward and leave inordinate room for competing explanators such as $\Delta d_{i,t-1}$. Indeed, in 15 of 17 cases in CRBB's core tables (numbered 4–9), moving from OLS in levels to OLS in differences increases the impact coefficients. Also, like all endogeneity, this should bias standard errors downward.

But a similar argument says that differencing renders the other predetermined variables, $d_{i,t-1}$ and $d_{i,t-1}^2$, presumptively endogenous too. That is, if we assume with CRBB that $d_{i,t-1}$ is endogenous to $\epsilon_{i,t-1}$ then we should expect the $d_{i,t-1}$ in $\Delta d_{i,t-1} = d_{i,t-1} - d_{i,t-2}$ to be correlated with the $\epsilon_{i,t-1}$ in $\Delta \epsilon_{it} = \epsilon_{it} - \epsilon_{i,t-1}$, producing an endogenous relationship of opposite sign between $\Delta d_{i,t-1}$ and $\Delta \epsilon_{it}$ in (2). The fundamental problem is that while lagging temporally separates $d_{i,t-1}$ from ϵ_{it} , differencing brings the measurement timeframes for $\Delta d_{i,t-1}$ and $\Delta \epsilon_{it}$ into overlap.

Roodman (2008) shows that growth negatively Granger-causes aid/GDP. The effect might run mechanically through the denominator: when GDP/capita goes up, GDP goes up, and aid/GDP goes down. As well, the numerator of the aid/GDP ratio may be endogenous to growth, since faster-growing countries may attract less aid. At any rate, the negative relationship from $d_{i,t-1}$ to $\epsilon_{i,t-1}$ should, by the math above, manifest as a positive relationship between $\Delta d_{i,t-1}$ and $\Delta \epsilon_{it}$ and should further inflate the aid/GDP coefficient estimates when regressing in differences.

A standard response to the way differencing renders the predetermined endogenous is to instrument the newly suspect variables with their lagged levels or differences. In the simplest implementation, this yields the Anderson-Hsiao (1982) levels and differences estimators, which are exactly identified instrumental variables estimators that can be implemented with Two-Stage Least Squares (2SLS). In the case at hand, we would instrument $\Delta y_{i,t-1}$, $\Delta d_{i,t-1}$, and $\Delta(d_{i,t-1}^2)$ with $y_{i,t-2}$, $d_{i,t-2}$, and $d_{i,t-2}^2$ or

$\Delta y_{i,t-2}$, $\Delta d_{i,t-2}$, and $\Delta(d_{i,t-2}^2)$ (Bond, Hoeffler, and Temple 2001). CRBB does the latter, presumably because this leads to stronger instrumentation.

But CRBB instruments $\Delta y_{i,t-1}$ only, not $\Delta d_{i,t-1}$ and $\Delta(d_{i,t-1}^2)$. It thus goes against the guidance of Bond (2002): “If x_{it} is assumed to be endogenous then it is treated symmetrically with the dependent variable y_{it} .” Table 4 illustrates why CRBB does not instrument aid. The table tests four instrument sets: $\Delta d_{i,t-2}$ and $\Delta d_{i,t-2}^2$; $d_{i,t-2}$ and $d_{i,t-2}^2$; $d_{i,t-3}$ and $d_{i,t-3}^2$; and $d_{i,t-2}$, $d_{i,t-2}^2$, $d_{i,t-3}$, and $d_{i,t-3}^2$.¹⁰ These combinations exhaust all natural possibilities when limiting the lag depth to three periods. The instruments prove uniformly weak. None of the tests produces a Kleibergen-Paap F statistic above 4. For the majority, we cannot even reject the null of underidentification. As expected—since weak instruments should cause bias toward OLS in differences, which should overestimate aid coefficients—the weakest identification leads to the largest coefficients. An analogous table for the revised specification tells the same story.¹¹

Although instrumenting aid is difficult, there is an alternative to the CRBB approach of doing nothing: double down on CRBB’s strategy for reducing endogeneity by *twice*-lagging aid when regressing in differences. Table 5 does that. Even though using deeper lags shrinks the sample, coefficients are estimated with similar precision as before (compare to Table 2). But now the implied aid impacts are *negative*. And the sign flips are not an artifact of a mismatch between the doubled lag between aid and growth and the time profile of early aid’s impacts: for negative signs appear on non-early aid too. (Once more, switching to the revised specifications leaves the pattern of results unchanged.)

It bears emphasizing that twice-lagging the aid/GDP variables is more than an econometric gambit: it is a change in structural model. The new model has the disadvantage of leaving out the central causal relationship in CRBB—from aid/GDP to GDP/capita one period later. But comments in that spirit cut both ways. CRBB’s preferred regressions leave out the two-period effect, the three-period effect, etc. They also exclude impacts transmitted (linearly) through the controls. The new regressions study a

different section of aid's impact, one that is no more or less canonical. The major difference is that this section can be studied more rigorously. Of all the regressions presented so far in CRBB and this paper, only those in Table 5 meet CRBB standards: they remove fixed factors such as population that could be associated omitted variable bias; and they temporally separate aid and growth to combat endogeneity.

Other Specification Issues and Fixes

Interpretations of the results presented so far can be strengthened by addressing two more econometric issues. The first is serial correlation, which is a first-order concern in panel econometrics and scrambles Granger inferences; it is not mentioned in CRBB despite the focus on timing. The second issue relates to how best to study nonlinearity in the aid-growth relationship. Just as CRBB introduce specification improvements absent from their subject studies, I make modifications to explore these issues. The appendix details and this section summarizes.

In an attempt to remove the serially correlated component of growth from the error I modify the regressions on twice-lagged aid by expanding the regressor sets to all quadratic combinations of controls, meaning $\text{vec}(\mathbf{xx}')$. This stratagem appears to help while leaving the results of interest intact. And taking a cue from CRBB, I perform semiparametric regressions. These also tend to confirm the parametric results. The next two figures illustrate. They form a five-graph sequence that starts from a specification that supports CRBB (CRBB Table 9, regression 9; OLS in levels on lagged early aid/GDP and its square). The sequence then introduces CRBB's specification changes and mine. Figure 1 starts. It shows the smoothed fit for the association between GDP/capita growth and lagged early aid/GDP after removing the linear effects of controls. It provides 95 percent confidence intervals for the semiparametric fit, and plots linear and quadratic fits. Over the bulk of the sample, on the left, the association is positive. It is statistically significant too, in that point estimates where early aid/GDP = 4–8 percent are above the confidence intervals where early aid/GDP = 0. And the quadratic fit more closely matches the nonparametric one over the majority of the data.

The sequence continues in Figure 2. First the data are differenced as in CRBB, producing that clear positive slope that is predicted to contain upward bias (corresponding to CRBB Table 8, regression 10). Then lagged growth is instrumented (CRBB Table 8, regression 11). Then aid variables are twice-lagged (as in the present Table 5, column 3). Then quadratic controls are added to reduce serial correlation (Table 6 in the appendix, column 5). The aid-growth association in this pane, the most rigorous in the sequence, is mildly negative over most of the sample.

CONCLUSION

CRBB's inference that aid probably causes growth does not appear well-founded in the CRBB data. Of CRBB's three timing-motivated specification changes, one—focusing on early aid—does not strengthen the aid-growth association. The other changes, when done rigorously by twice-lagging aid, point to an association that is zero or negative. And even if the CRBB regressions are taken as reasonably valid, they do not produce much evidence of positive impact. Even the maximum impact is rarely statistically significant.

Though I focus on CRBB, I do not see it as particularly unreliable. Easterly, Levine, and Roodman (2004) finds fragility in BD. Bazzi and Clemens (2009) documents significant problems in RS: the cross-section regressions depend on the dubious excludability of population size; the instruments in the panel GMM regressions are weak. Even leaving aside all the econometric issues discussed here and in CRBB, fundamental questions of identification remain in all observational cross-country studies. Variables such as GDP are poorly measured. Controls too may be endogenous. Direct econometric evidence of the impact of aid on growth may be a bridge too far.

Appendix: Improving the Specifications

Here I discuss and attempt to resolve two specification issues raised in Roodman (2008), as they pertain to CRBB.

Serial Correlation

Arellano and Bond (1991) is the most-cited paper in panel econometrics. Though viewed as the source of an estimator, its purpose is to develop and assess specification tests for already-established estimators in the Anderson-Hsiao family. Of particular concern is serial correlation, which undermines consistency when instrumenting with lags. With reference to (2) in the main text, if there is first-order serial correlation, then the $y_{i,t-2}$ in the instrument $\Delta y_{i,t-2} = y_{i,t-2} - y_{i,t-3}$ may be presumed correlated not just with $\epsilon_{i,t-2}$ but also with the $\epsilon_{i,t-1}$ in the error $\Delta \epsilon_{it}$. Arellano and Bond introduce a z-statistic (normally distributed) test for serial correlation in short panels with cross-sectionally independent errors. When a specification fails the test, researchers should consider instrumenting with deeper lags instead and then testing for longer-order serial correlation.

Lack of serial correlation is also essential to Granger's conception of causality, which CRBB takes as its guide star. Granger's feasible test, for example, assumes white noise errors. Despite focusing on timing effects, CRBB does not mention the issue. Nor, in most cases, does the study adjust standard errors by clustering by country as in BD, RS, and CRB.

As documented in the main text, serial correlation is prevalent in the CRBB regressions and in my extensions of them, the biggest exceptions being the BD regressions with twice-lagged aid (left half of Table 5).

At least for the RS regressions, adding all quadratic combinations of controls— $\text{vec}(\mathbf{xx}')$ as well as $\mathbf{x}$ —extracts much of the persistent component of growth from the error. As in (1), the controls $\mathbf{x}$ here exclude time dummies and GDP/capita. Table 6 demonstrates: it modifies Table 5 in adding quadratic controls, and in incorporating the revised specifications. The finding persists of a mild, negative aid-growth association over two periods. The most rigorous parametric impact estimates in this paper are those for BD in Table 5, where serial correlation is not clearly detected in the first place, and for RS in Table 6, where serial correlation is removed.¹²

Linear, Quadratic, and Semiparametric Estimation

Roodman (2008) worries that the use of quadratic aid terms in CRB, Hansen and Tarp (2001), and other aid-growth papers interacts problematically with outlier observations. To rebut this concern, CRBB moves toward semiparametric analysis. It is a fruitful direction. On balance, semiparametric analysis backs CRBB's assertion that their results are not a fragile artifact of an interaction between the quadratic specification and outliers.

Perhaps the best way to put the Roodman (2008) concern about quadratic specifications is this: aid variables are always or nearly always positive. That makes them nearly multicollinear with their squares. In regressions, identification of the coefficients on these linear and quadratic aid terms arises from the (small) unique components of variation in each (Angrist and Krueger 1999, 1310–14). A regression will then identify the Local Average Treatment Effect (LATE; Imbens and Angrist 1994) of aid/GDP and $(\text{aid/GDP})^2$ for these small components of variation associated with extreme observations. If the regression functional form is precisely correct—if parameters are homogenous—then extrapolation of the LATE to the full sample will be consistent. But in the messy world of cross-country econometrics, the model is unlikely to be exactly right. The smaller the identifying variation and the more extreme the observations that generate most of it, the greater the risk that the LATE will be misleading. In short, parabolic fits can be sensitive to outliers.

In response, CRBB presents a scatter plot. Referring to equation (2), but suppressing Δ 's for clarity, its procedure can be described as partialling x linearly out of y and d_{t-1} , then plotting the residuals $P_x^\perp y$ against the residuals $P_x^\perp d_{t-1}$. For the selected regression, CRBB's plot reassures with an apparently strong positive association not dominated by a few outliers.

This analysis appears to contain three significant problems. First, semiparametric analysis requires more than a scatter plot. Patterns that appear obvious to the naked eye may not be statistically

significant. This is why semiparametric analysis entails regression too, with the familiar tools of point estimates and confidence intervals. Second, an apparent coding error causes CRBB to plot $P_x^{\parallel}y$ and $P_x^{\parallel}d_{t-1}$, the components of y and d parallel rather than orthogonal to $\mathbf{x}$.¹³ Third, $P_x^{\perp}y$ and $P_x^{\perp}d_{t-1}$ are a nonstandard basis for semiparametric analysis. A more established approach for partially linear models such as CRBB's is Robinson's (1988). It works as follows. We wish to fit the equation:

$$y = \boldsymbol{\gamma}'\mathbf{x} + f(d) + \epsilon \quad (3)$$

where $\mathbf{x}$ and d are exogenous to the error ϵ and f is an unknown function. We take expectations conditional on d :

$$E[y|d] = \boldsymbol{\gamma}'E[\mathbf{x}|d] + f(d)$$

and subtract them from (3):

$$y - E[y|d] = \boldsymbol{\gamma}'(\mathbf{x} - E[\mathbf{x}|d]) + \epsilon \quad (4)$$

We estimate $E[y|d]$ and $E[\mathbf{x}|d]$ using nonparametric regression, such as a kernel-weighted moving average, producing $E_N[y|d]$ and $E_N[\mathbf{x}|d]$. This leads to a feasible estimator of $\boldsymbol{\gamma}$ in (4): we regress $y - E_N[y|d]$ on $\mathbf{x} - E_N[\mathbf{x}|d]$ using OLS, producing $\hat{\boldsymbol{\gamma}}$. Returning to (3), we estimate f with nonparametric regression of $y - \hat{\boldsymbol{\gamma}}'\mathbf{x}$ on d .

We can generalize the procedure to the case when $\mathbf{x}$ is endogenous and enough instruments $\mathbf{z}$ are available. Then, we estimate $\boldsymbol{\gamma}$ in (4) with a linear GMM estimator based on $\mathbf{z}$. The regressions of interest here are exactly identified, so GMM estimators coincide and we implement with 2SLS.

In contrast, CRBB move toward studying $E[P_x^{\perp}y|P_x^{\perp}d]$. This is intuitive, but by the assumptions above, $E[P_x^{\perp}y|P_x^{\perp}d] = E[P_x^{\perp}(\boldsymbol{\gamma}'\mathbf{x} + f(d) + \epsilon)|P_x^{\perp}d] = E[P_x^{\perp}f(d)|P_x^{\perp}d] + E[\epsilon|P_x^{\perp}d] = E[P_x^{\perp}f(d)|P_x^{\perp}d]$. This is not $E[f(P_x^{\perp}d)|P_x^{\perp}d] = f$, the function of interest, if f is nonlinear.

Figure 3 applies Robinson's procedure to the specification behind CRBB's scatter plot (a BD-based OLS regression in differences on net ODA). In the first stage of the procedure, $E_N[y|d]$ and

$E_N[\mathbf{x}|d]$ are estimated as moving-average fits with Stata's "lpoly" command, taking defaults (Epanichnikov kernel, Stata's rule-of-thumb algorithm for setting bandwidth). The second-stage regression, shown, is done the same way. It is formatted like Figure 1, with 95 percent confidence intervals for the smoothed fit, and linear and quadratic fits as well.

The figure supports CRBB in suggesting a significant, positive aid-growth association in this specification over the bulk of the sample. Point estimates toward the right of the main data mass are above the confidence intervals toward the left, or nearly so, and vice versa. And the figure shows that this quadratic fit does not wildly chase outliers. That said, the overall relationship is well captured by the linear fit too.

Similar graphs for the RS early aid specifications are in the main text. As noted, they line up well with the parametric results. In some cases, such as the second pane of Figure 2, the parabola appears to fit the data better than the line over the main data mass. In others, such as in the last pane, outliers appear to dominate the quadratic fit. Overall, the semiparametric regressions provide a useful robustness test for the parametric ones.

REFERENCES

- Anderson, T.W. and Chen Hsiao. 1982. Formulation and Estimation of Dynamic Models Using Panel Data. *Journal of Econometrics* 18 (1): 47-82.
- Angrist, Joshua D. and Krueger, Alan B. 1999. Empirical Strategies in Labor Economics. In O. C. Ahsenfelter and D. Card, eds., *Handbook of Labor Economics* 3. Amsterdam, The Netherlands: Elsevier B. V. – North Holland Publishers.
- Bazzi, Samuel and Michael A. Clemens. 2009. Blunt Instruments: A Cautionary Note on Establishing the Causes of Economic Growth. Working Paper 171. Washington, D.C.: Center for Global Development.
- Bond, Stephen. 2002. *Dynamic Panel Data Models: A Guide to Microdata Methods and Practice*. London, UK: Centre for Micro Data Methods and Practice.

- Bond, Stephen R., Anke Hoeffler, and Jonathan Temple. 2001. GMM Estimation of Empirical Growth Models. Discussion Paper 3048. London, UK: Centre for Economic Policy Research.
- Boone, Peter. 1996. Politics and the Effectiveness of Foreign Aid. *European Economic Review* 40 (2): 289-329.
- Burnside, Craig and David Dollar. 2000. Aid, Policies, and Growth. *The American Economic Review* 90 (4): 847-868.
- Clemens, Michael A., Steven Radelet, and Rikhil R. Bhavnani. 2004. Counting Chickens When They Hatch: The Short-term Effect of Aid on Growth. Working Paper 44. Washington, D.C.: Center for Global Development.
- Clemens, Michael A., Steven Radelet, Rikhil R. Bhavnani, and Samuel Bazzi. 2012. Counting Chickens When They Hatch: Timing and the Effects of Aid on Growth. *Economic Journal* 122: 590-617.
- Easterly, William, Ross Levine, and David Roodman. 2004. Aid, Policies, and Growth: Comment. *The American Economic Review* 94 (2): 774-780.
- Frankel, Jeffrey A. and David Romer. 1999. Does trade cause growth? *The American Economic Review* 89 (3): 379-399.
- Granger, Clive W. J. 1969. Investigating Causal Relations by Econometric Models and Cross-Spectral Methods. *Econometrica* 37 (3): 424-438.
- Hansen, Henrik and Finn Tarp. 2001. Aid and Growth Regressions. *Journal of Development Economics* 64 (2): 547-570.
- Hausmann, Ricardo, Jason Hwang, and Dani Rodrik. 2007. What you export matters. *Journal of Economic Growth* 12 (2): 1-25.
- Imbens, Guido W. and Joshua D. Angrist. 1994. Identification of Local Average Treatment Effects. *Econometrica* 62 (2): 467-475.

- Judson, Ruth A. and Ann L. Owen. 1999. Estimating Dynamic Panel Models: A Practical Guide for Macroeconomists. *Economics Letters* 65 (1): 9-15.
- Kleibergen Frank and Richard Paap. 2006. Generalized Reduced Rank Tests using the Singular Value Decomposition. *Journal of Econometrics* 127 (1): 97-126.
- Rajan, Raghuram G., and Arvind Subramanian. 2008. Aid and Growth: What Does the Cross-Country Evidence Really Show? *The Review of Economics and Statistics* 90 (4): 643-665.
- Robinson, P.M. 1988. Root-N-Consistent Semiparametric Regression. *Econometrica* 56(4): 931-954.
- Roodman, David. 2007. The Anarchy of Numbers: Aid, Development, and Cross-Country Empirics. *World Bank Economic Review* 21 (2): 255-277.
- Roodman, David. 2008. Through the Looking Glass, and What OLS Found There: On Growth, Foreign Aid, and Reverse Causality. Working Paper 137. Center for Global Development.
- Temple, Jonathan. 1999. The New Growth Evidence. *The Journal of Economic Literature* 37 (1): 112-156.

ENDNOTES

¹ For economy of language, I sometimes use “impact” to describe an aid-growth association, without intending to imply an interpretation of causality from aid to growth.

² While helpful, this classification can blur the distinction between design and inference. RS, for example, runs “conditional” regressions with “null” results.

³ RS also perform long-period cross-section regressions, but these receive less attention, probably because CRBB tend to favor panel regressions, which allow more careful handling of timing effects.

⁴ The original Boone sample, covering 1971–1990 with 10-year periods, does not support all of CRBB’s specification changes. Lagged, differenced values of early aid/GDP for this sample require observations

of early aid in the 1950s and 1960s, which CRBB cannot estimate. However, CRBB's extended 1971–2000 sample *does* support all of the changes since CRBB estimates early aid for the 1970s. For reasons not stated, CRBB takes advantage of this extension only to perform OLS in differences with early aid/GDP, not Anderson-Hsiao. The cause may be Boone's peculiar dynamic specification, which includes not just GNI/capita, but its square too. Jointly, the two terms are extremely weakly instrumented by their 10-year lags, obstructing Anderson-Hsiao (Kleibergen-Paap test for underidentification $p = 0.52$). A potential fix—dropping the squared term—still leaves the instrument weak (Kleibergen-Paap test for underidentification $p = 0.14$, Kleibergen-Paap F statistic = 2.74). Moreover, the extended Boone panel is too short for an important specification change introduced below, the use of twice-lagged aid variables. For all these reasons, I follow CRBB in setting aside Anderson-Hsiao adaptation of the Boone specification.

⁵ For the BD regressions, the added countries are Barbados, Burkina Faso, Burundi, Chad, China, Republic of Congo, Cyprus, Iran, Israel, Jordan, Lesotho, Liberia, Malta, Mauritania, Mauritius, Nepal, Panama, Papua New Guinea, Rwanda, Singapore, South Africa, Uganda, and Yemen. For the RS regressions, the added countries are Albania, Bangladesh, Barbados, Burundi, Chad, Croatia, Guatemala, Guinea-Bissau, Honduras, Lesotho, Liberia, Mali, Malta, Mauritania, Mauritius, Nepal, Rwanda, Slovenia, Swaziland, Tanzania, Yemen, and Zambia. In the latter case, some of the countries are in the original study's panel (RS, Appendix 2).

⁶ BD uses Newey-West standard errors of undocumented maximum lag length.

⁷ Adding $y_{i,t-1}$ to both sides of (1) yields an AR(1) form:

$$y_{it} = \alpha y_{i,t-1} + \beta_1 d_{i,t-1} + \beta_2 d_{i,t-1}^2 + \mathbf{Y}' \mathbf{x}_{it} + \eta_t + \nu_i + \epsilon_{it}$$

⁸ CRBB also control for reflows on aid loans and treat these as predetermined. I leave these variables out of the discussion for simplicity but in the empirics treat them in the same way as I treat d .

⁹ This is more easily seen in the equation in note 7.

¹⁰ Throughout, repayments/GDP and its square are instrumented analogously.

¹¹ Excluding the quadratic aid terms from these regressions does not help the situation much either.

¹² Parallel tables for the “revised” specifications are in the spreadsheet on this paper’s web page.

¹³ In lines 554 and 556 of the public main_regs_final.do Stata program, the “predict” commands lack a “residuals” option.

Table 1. Replication of CRBB Preferred-Specification Regressions

	Burnside & Dollar		Rajan & Subramanian	
	Original	Replication	Original	Replication
Estimator	A-H	A-H	A-H	A-H
Aid lagged?	Yes	Yes	Yes	Yes
First difference?	Yes	Yes	Yes	Yes
Early-impact aid?	Yes	Yes	Yes	Yes
Extended years?	Yes	Yes	Yes	Yes
Early aid/GDP, lagged	0.324 (0.316)	0.324 (0.316)	0.513 (0.384)	0.513 (0.384)
(Early aid/GDP) ² , lagged	-3.6* (2.0)	-3.567* (2.003)	-4.2 (2.9)	-4.239 (2.927)
Initial log real GDP/capita		-0.204*** (0.076)		-0.130* (0.071)
Maximum-impact aid level	0.0455 (0.0315)	0.0455 (0.0315)	0.0605*** (0.0179)	0.0605*** (0.0179)
Maximum impact		0.007 (0.012)		0.015 (0.014)
Years/period	4		5	
Timeframe	1978–2005		1981–2005	
Countries	54		62	
Observations	323		268	

Results for controls, including log initial GDP/capita, not shown. Maximum-impact aid level and maximum impact are the coordinates of the extremum of the parabola given by the coefficients on Aid/GDP and (Aid/GDP)²; for the parabola $y = ax^2 + bx$, they are $(-b/2a, -b^2/4a)$. Heteroskedasticity-robust standard errors in parentheses. *significant at 10%. ***significant at 1%.

Table 2. Initial Specification and Robustness Checks

	Burnside & Dollar		Rajan & Subramanian	
	Replication	Revised	Replication	Revised
Early aid/GDP, lagged	0.324 (0.316)	0.230 (0.245)	0.513 (0.384)	0.142 (0.277)
(Early aid/GDP) ² , lagged	-3.567* (2.003)	-3.459** (1.616)	-4.239 (2.927)	-1.988 (1.425)
Maximum-impact aid level	0.0455 (0.0315)	0.0332 (0.0249)	0.0605*** (0.0179)	0.0357 (0.0482)
Maximum impact	0.007 (0.012)	0.004 (0.007)	0.015 (0.014)	0.003 (0.008)
Kleibergen-Paap underidentification test (<i>p</i>)	0.00	0.00	0.02	0.04
Kleibergen-Paap <i>F</i>	23.81	9.78	4.94	4.08
Arellano-Bond <i>z</i>	1.51	1.76*	-2.15**	-0.81
Years/period	4	4	5	5
Timeframe	1978–2005	1978–2005	1981–2005	1981–2005
Countries	54	77	60	82
Observations	323	439	268	337

Results for controls, including log initial GDP/capita, not shown. Anderson-Hsiao (A-H) regressions instrument lagged growth with twice-lagged growth. Revised regressions add all available observations in CRBB datasets, fix an apparent error in the M2/GDP variable in the RS data set, and cluster standard errors by country. Arellano-Bond test is for second-order correlation in differences. Standard errors, in parenthesis, are heteroskedasticity-robust in the replication regression and country-clustered in the revised ones. *significant at 10%. **significant at 5%. ***significant at 1%.

Table 3. Regressions on Early and Non-early Aid

	Burnside & Dollar			Rajan & Subramanian		
Early aid/GDP, lagged	0.324 (0.316)		0.244 (0.357)	0.513 (0.384)		0.579 (0.473)
(Early aid/GDP) ² , lagged	-3.567* (2.003)		-2.914 (2.166)	-4.239 (2.927)		-4.920 (3.986)
Non-early aid/GDP, lagged		0.310 (0.295)	0.225 (0.327)		0.082 (0.246)	-0.042 (0.267)
(Non-early aid/GDP) ² , lagged		-0.686 (0.799)	-0.449 (0.897)		-0.264 (0.808)	0.304 (0.937)
Maximum-impact aid level	0.045 (0.031)	0.226*** (0.083)		0.060*** (0.018)	0.155 (0.228)	
Maximum impact	0.007 (0.012)	0.035 (0.030)		0.015 (0.014)	0.006 (0.023)	
Kleibergen-Paap underid (<i>p</i>)	0.00	0.00	0.00	0.02	0.01	0.02
Kleibergen-Paap <i>F</i>	23.81	29.44	26.23	4.94	6.47	5.14
Arellano-Bond <i>z</i>	1.51	1.50	1.59	-2.15**	-2.31**	-2.33**
Observations	323	323	323	268	268	268

Results for controls, including log initial GDP/capita, not shown. Regressions instrument lagged growth with twice-lagged growth. Arellano-Bond test is for first-order serial correlation in levels, for OLS, and second-order correlation in differences, for A-H. Heteroskedasticity-robust standard errors in parenthesis. *significant at 10%. **significant at 5%. ***significant at 1%.

Table 4. Anderson-Hsiao Regressions with Various Instrument Sets

Instruments for aid (Δd_{t-1}):	Burnside & Dollar				Rajan & Subramanian			
	Δd_{t-2}	d_{t-2}	d_{t-3}	d_{t-2}, d_{t-3}	Δd_{t-2}	d_{t-2}	d_{t-3}	d_{t-2}, d_{t-3}
Early aid/GDP, lagged	2.871 (1.982)	1.004 (1.481)	6.565 (8.991)	2.047** (1.039)	-2.134 (9.379)	1.720* (1.030)	2.161 (2.260)	1.667** (0.816)
(Early aid/GDP) ² , lagged	-10.513 (8.435)	-6.713 (5.939)	-54.378 (72.102)	-10.167** (4.300)	33.615 (108.278)	-7.425 (8.577)	-11.543 (32.475)	-11.608* (7.024)
Maximum-impact aid level	0.137*** (0.031)	0.075 (0.059)	0.060* (0.035)	0.101*** (0.021)	0.032 (0.043)	0.116 (0.085)	0.094 (0.170)	0.072*** (0.016)
Maximum impact	0.196 (0.126)	0.038 (0.082)	0.198 (0.323)	0.103 (0.067)	-0.034 (-0.192)	0.100 (0.068)	0.101 (0.096)	0.060** (0.028)
Kleibergen-Paap underid p	0.06	0.00	0.41	0.02	0.65	0.00	0.77	0.00
Kleibergen-Paap F	0.79	1.95	0.14	2.33	0.04	2.12	0.02	3.24
Arellano-Bond z	-1.47	1.52	0.36	0.11	-0.22	-2.92***	-0.18	-2.35**
Hansen J (p value)				0.19				0.27
Observations	276	323	276	276	214	268	214	214

Results for controls, including log initial GDP/capita, not shown. Arellano-Bond test is for first-order serial correlation in differences. Hansen J tests for instrument invalidity. Heteroskedasticity-robust standard errors in parenthesis.

*significant at 10%. **significant at 5%. ***significant at 1%.

Table 5. Anderson-Hsiao Regressions on Twice-lagged Aid/GDP

	Burnside & Dollar		Rajan & Subramanian	
Early aid/GDP, twice-lagged	−0.339		−0.483	
	(0.307)		(0.365)	
(Early aid/GDP) ² , twice-lagged	0.291		3.614	
	(1.753)		(3.504)	
Non-early aid/GDP, twice-lagged		−0.404*		−0.254
		(0.226)		(0.209)
(Non-early aid/GDP) ² , twice-lagged		1.314**		−0.309
		(0.603)		(0.830)
Maximum-impact aid level	0.582	0.154***	0.067**	−0.411
	(3.055)	(0.042)	(0.026)	(−1.417)
Maximum impact	−0.099	−0.031	−0.016	0.052
	(−0.446)	(−0.023)	(−0.012)	(0.221)
Kleibergen-Paap underid (<i>p</i>)	0.00	0.00	0.00	0.00
Kleibergen-Paap <i>F</i>	41.69	44.28	30.95	23.90
Arellano-Bond <i>z</i>	0.10	0.16	−1.75*	−1.86*
Observations	276	276	215	215

Results for controls, including log initial GDP/capita, not shown. Regressions instrument lagged growth with twice-lagged growth. Arellano-Bond test is for second-order correlation in differences. Heteroskedasticity-robust standard errors in parenthesis. *significant at 10%. **significant at 5%. ***significant at 1%.

Table 6. Anderson-Hsiao Regressions on Early vs. Non-early Impact Aid and Quadratic Controls

	Burnside & Dollar				Rajan & Subramanian			
	Replication	Revised	Replication	Revised	Replication	Revised	Replication	Revised
Early aid/GDP, twice-lagged	−0.396 (0.338)	−0.101 (0.255)			−0.202 (0.373)	−0.342 (0.356)		
(Early aid/GDP) ² , twice-lagged	0.651 (2.019)	0.007 (1.410)			0.246 (3.694)	2.673 (3.051)		
Non-early aid/GDP, twice-lagged			−0.546** (0.268)	−0.381* (0.197)			−0.301 (0.227)	−0.420 (0.274)
(Non-early aid/GDP) ² , twice-lagged			1.710** (0.674)	1.043** (0.404)			−0.068 (0.944)	0.985 (0.785)
Maximum-impact aid level	0.304 (0.721)	6.816 (1283.997)	0.160*** (0.036)	0.183*** (0.029)	0.411 (5.493)	0.064** (0.032)	−2.230 (−32.699)	0.214*** (0.051)
Maximum impact	−0.060 (−0.106)	−0.343 (−63.914)	−0.044 (−0.028)	−0.035 (−0.023)	−0.042 (−0.489)	−0.011 (−0.013)	0.336 (5.162)	−0.045* (−0.026)
Kleibergen-Paap underid (<i>p</i>)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Kleibergen-Paap <i>F</i>	33.09	12.32	34.59	10.70	13.17	16.05	9.23	13.20
Arellano-Bond <i>z</i>	−0.35	0.84	−0.27	1.50	−0.73	−0.83	−0.79	−0.58
Observations	276	381	276	381	215	269	215	269

Results for controls, including log initial GDP/capita, not shown. Regressions instrument lagged growth with twice-lagged growth. Arellano-Bond test is for second-order correlation in differences, for A-H. Standard errors, in parenthesis, are heteroskedasticity-robust in the replication regression and country-clustered in the revised ones. *significant at 10%. **significant at 5%. ***significant at 1%.

Figure 1. Semiparametric Regression of GDP/capita Growth on Early Aid/GDP, in Context of RS Specification (CRBB Table 9, Regression 9)

Figure 2. Semiparametric Regression of GDP/capita Growth on Early Aid/GDP, in Context of RS Specification (CRBB Table 9, Regressions 10–11)

Figure 3. Semiparametric Regression of GDP/capita Growth on Net ODA/GDP, in Context of BD Specification (CRBB Table 7, Regression 7)